Afterword: Evidence over Interests

John P. Gluck
Professor Emeritus, University of New Mexico; Research Professor, Kennedy Institute of Ethics, Georgetown University, United States
jgluck@unm.edu

After years of science education, teaching experience, and research practice, which focused on the use of non-human primates as potential models of human psychological disorders, a young student in my primate behavior class amiably, but insistently, suggested my preparation was incomplete. She asked me to read Peter Singer's book *Animal Liberation*, which had been published two years earlier, in 1975. I had been lecturing in class about the effects of early experience on the rhesus monkey's (*Macaca mulatta*) social and intellectual development, and my descriptions of the invasive research interventions and behavioral consequences encouraged her to make the book suggestion. I said I would try to find the time, but that I was busy. She handed me a fresh new copy of the book saying, "This is for you." She made it clear that she was not loaning me her copy but wanted the book to be part of my professional library. Over the following weeks while describing this event to colleagues, many also involved in animal research, I asked them if they had read Professor Singer's book. While some had heard of it, no one had actually read it. "Why should I do that?" was a common tone of the comments. After all, our experimental standards were quite clear and seemed self-evidently valid. That is, if any interesting and, therefore, valuable research question could not be tested in humans for ethical reasons, then it could be evaluated in animal models. Progress required risk, and progress was urgently needed. This powerful drive to know and understand nature, so as to improve the welfare of human beings, was what the bioethicist Paul Ramsey (1976) called, the *research imperative*, to emphasize its motivational dominance.

In response to the student's questioning looks as we saw one another in class, and out of respect for her serious intention, I did finally read *Animal Liberation*. The chapter titled, *Tools for research or what the public doesn't know it is paying for*, quickly trapped my attention. Three of the assertions of the chapter were: (1) The raw descriptions of the experimental manipulations done to animals revealed a shocking emotional callousness on the part of investigators; (2) The extent of the harms, which the animals were required to absorb, seemed excessive in comparison with the many obvious or even trivial facts discovered; (3) It was estimated that after all the experimental effort and

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animal suffering, approximately one quarter of the studies actually made it into the open scientific literature. I thought the number was significantly less than 25%. More personally, a significant part of the chapter raised specific ethical questions about the research of Harry F. Harlow, which also involved studying the effects of socially isolating infant rhesus monkeys from their mothers and peers. Harlow was once one of my central mentors in graduate school and continued to support me by providing monkey subjects and experimental advice. Singer described the laboratory where I was educated, and he sounded morally disgusted. While I mostly rejected the implication, it was clear that the basic assertion of *Animal Liberation* was that our vague and rarely articulated ethical assumptions, when placed under the light of a sophisticated utilitarian ethical analysis, revealed themselves to be simplistic, self-serving, and mostly indifferent to science-generated animal harm and suffering.

As my colleagues and I began to see an increase in the number of pointed questions about the validity and justification of animal models from students, a few scientists, and from the public after 1975, curiosity about the controversy and the issues raised turned to hardened defensiveness and something approaching contempt for the questioners. We bolstered our dismissiveness by making forceful statements about the demands of the research imperative and the extent of human clinical need. We remained blind or just mute about the dangers that can accrue when an imperative becomes an omnipotent and unassailable directive.

More subtle, but perhaps more dangerous, is that there is evidence that many researchers have neglected and continue to neglect the notion that science is not just based on acts of direct perception of nature, followed by the straightforward description of facts. Rather, it is a process strongly influenced by psychological, social, and cultural forces. Ludwik Fleck, a microbiologist who wrote as early as 1935 about how different sides of many scientific controversies evolved into *thought collectives* that demanded loyalty to the beliefs of the collective and disdain for outsiders. Fleck showed that the collectives were capable of shaping *thought styles* that could have the effect of limiting the ability of members to actually understand divergent perspectives and to take alternative research paths. The magisterial work of Thomas Kuhn (1962), in *The Structure of Scientific Revolutions,* which built on Fleck (1935), articulated how, what he termed, "normal science" could actively deny incorporating experimental findings that had the potential to disconfirm entrenched methods and explanations. More recently, historians of science, such as David Wootton (2007) in his book *Bad Medicine: Doctors Doing Harm since Hippocrates,* further elaborate how the tendency of scientists to confer authority to "established" theories and methods have been the central factor in the delay of medical progress, and so it is now with much of the work in animal research.